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TENDENCIES IN ECONOMIC THEORY—DISCUSSION

H. J. DAVENPORT: I find my place in the discussion of these papers a difficult one. Professor Hollander's paper urges—if I understand it rightly—that little or nothing worth while has been accomplished in theory during the last thirty years—there being little really left to do. Professor Mitchell, on the contrary, holds that the accomplishment is great and admirable, only that the process must go much farther in the same general direction. I should perhaps do well to withdraw and to let these men settle the matter between themselves, rather than to take an intermediate position involving the mugwump hazard of getting shot into from both directions.

In the main, however, I concur in the position of Professor Hollander. Had it been within my power—and had it appeared to me worth while—I should have written much this same sort of paper, trying possibly to improve on the temper of it, but ratifying and emphasizing the well-known essentials of all scientific method,—wise induction, accurate deduction, adequate verification. I agree that whenever wrong conclusions have been reached, something must have been amiss in one or more of these processes. Perhaps, however, I should have emphasized a further process; for it seems to me clear that before one has arrived at the point of making any induction, he must have had in mind some query or interest or problem, on the basis of which to select and group the facts appealing to his attention. If there be in his mind no principle of grouping—of determining what, among the infinitude of things to be seen, he shall see and shall make the basis of the inductive process—he will see nothing in particular, and have nothing from which to induce. But suppose him somehow to have selected, say, a chicken, a harvesting machine, a sofa-pillow, and a pound of Stilton cheese, as his facts to be thought about. Even in making this grouping, he must at least have decided that featheredness is not the organizing interest, because only the chicken and the cushion are feathered. Now assume that he notes that they all bear prices—aware, of course, all the while that the possession of feathers cannot be the explanation—and so induces the hypothesis that labor content must explain not only the prices but also the price ratios between the articles. Now let him proceed to deduce from this law,—this tentative, hypothetical explanation. If he deduce logically, a difficulty must present itself with his hypothesis immediately that he comes upon something that bears a price, e.g., land, the existence and the price of which can have involved no labor. Forthwith, his hypothesis must, some-

how, perhaps through further induction, be modified or reconstructed. If he does neither of these things, his process of verification has been defective—has not consulted facts enough—lacks due verification. Continually emerging discrepancy is the very nature of thinking, as the pragmatists tell us.

Thus, I go along, in the main, with Professor Hollander's views upon right method. But I am compelled to disagree with him in his choice of the writers and the doctrines that illustrate right method. He is sure that the later theorists have erred somewhere, and is sure of this precisely because he does not concur in the conclusions which they have reached. His test of method is one with yours and mine,—are the results correct? If you have arrived at right results—as you have—their criticism cannot touch you. Nor do I regard myself as falling under the strictures, since whatever I hold as correct, I have to believe, for the time being. Nor, by this test, can Professor Hollander suspect his own methods. So, I say, let the whip sing about the legs of the other fellows,—who are wrong. While they dance you and I may be comfortable. Let the galled jade wince; my withers—yours, also—are unwrung. So, if Professor Hollander is satisfied to believe that the law of increasing returns, rightly induced, deduced, and verified, is merely the obverse of the law of diminishing returns, or that he may hold that any or all of the five different sorts of land differentials can have no part in price determination, or that there are the indicia of right scientific method in Cairnes's doctrine of non-competing groups—where, if the members are in one vocation, they compete but get wages disproportional to pains, and if in different vocations, may get proportional wages but cannot compete—he is justified in his appraisal of the methods by his appraisal of the conclusions. I, in turn, regarding the conclusions as mostly wrong, find in precisely these cases admirable examples of just the sort of methods to avoid.

In this sense, then, I am glad to find myself in full accord with Professor Hollander's fundamentals, and in disagreement only in the minor matter of applications. And on this basis, also, as I take it, may Professor Mitchell easily find his place in the discussion. But he and I are also in substantial agreement on the question of where the right conclusions are to be found, and upon the further directions in which still more will be discovered. But I do not altogether like it of him that he declares me to belong to the psychological school, despite the unimportant fact mentioned by him that I declare that I do not so belong. No matter, however; for I don't precisely know what it means to be a member of this school. I feel much as did Billy Baxter,

when someone called him a duffer. He said that he did not know at all what a duffer might be, but judged it, anyway, to be about the worst kind of a thing possible for a fellow to be.

W. H. HAMILTON: In the two papers to which we have just listened an issue in economic methodology is clearly joined. Professor Hollander has given a spirited plea for "anti-intellectualism." Professor Mitchell has presented a careful argument for rationalism. Quite paradoxically the former has advanced a speculative argument against "theorizing," while the latter has presented by concrete instance, and hence inductively, a defense of theory.

In Professor Hollander's paper two main theses stand out. The first is a denial of validity to the methods of verification which economic theorists have employed. Since it is a reduction of the phenomena of a particular field to generalizations, it convicts Professor Hollander of "theorizing." But it is, in his words, a "hypothesis," rather than a "theory" or a "law," since he has failed to verify it. He has ignored the voluminous literature of methodology, possessed not only by economics but by the other social sciences. He has overlooked the examination of the nature of economic problems and phenomena found therein, and the attempts to establish methods of verification in harmony with these. This omission would have been pardonable, had he given attention to this issue. Instead he has been content to verify his conclusions solely by analogy. He argues that because certain methods have yielded valuable results in the natural sciences (which, it might be added, are still in the descriptive stage), economists should have used these methods of verification. One of the most valuable parts of his paper is his protest against the use of analogy. It would accordingly be unfair to him to accept his conclusions, so long as they are supported alone by analogical verification.

His second point is an imputation of almost negligible value to generalization and speculation. Again he convicts himself, for the paper just read abounds in generalization and in what I should regard as very adventurous speculation. The issue is squarely presented in his strictures upon the writing of textbooks. The guilty among us scarcely need defense at my hands. I wish only to remark that such general and speculative writing serves two closely related and indispensable functions: first, it allows us to take tentative stock of our accomplishments and lack of them; and, second, it reveals our shortcomings and points to the tasks which are most worth the doing. A piece of work cannot confer value upon itself. If carefully, and "scientific-

cally," done, it is not, for that reason, necessarily valuable. Above all it can furnish no evidence that it represented the task most worth doing; that the resources spent upon it might not have yielded more valuable results if employed otherwise. Its value must come from some external source; its justification must be found in the uses to which it can be put.

It is just such an evaluation of results and of possible tasks which textbooks in economics (which are descriptions of coherent systems rather than mere pedagogical manuals) have furnished. To take a concrete example, just now the subject of trade unionism is badly in need of a general, even if tentative, statement of results. The clear vision and the sense of relative values which such a text would impart should suggest to the workers in the field the tasks which are really most worth doing. It should prevent the production of such a large amount of mediocre work, and the waste of valuable labor in many futile inquiries. I cannot forbear in this connection repeating, with, I trust, a pardonable change of emphasis, the quotation from Huxley with which Professor Hollander, quite happily but somewhat inadvertently, closed his paper: "*Whenever science has halted, or strayed from the right path, it has been, either because its votaries have been content with unverified or unverifiable speculation, or it has been because the accumulation of details of observation has for a time excluded speculation.*"¹

With the methodological thesis implicit in Professor Mitchell's paper I am in agreement. The task of economic theory in balancing observation, generalization, and speculation is an extremely difficult one. Observation is necessary to relevancy; generalization to consistency. The subject has ever been, and in its nature must ever remain, the center of a perpetual struggle between relevancy and consistency. The economic system is rapidly changing, as is the intellectual system, in terms of which the explanation is to be made. Yet consistency can come only from temporarily arrested observation; the careful and tortuous work necessary to it requires decades of "hard thinking."

The chief criticism of the prevalent neo-classical economics is that it enjoys the latter at the expense of the former. Yet recently the note of relevancy has been quite dominant. The development of theory is just now being characterized by several more or less independent tendencies. As yet they present no semblance of articulation into a coherent system. These tendencies are semi-critical, semi-constructive. Among the former perhaps the most important is the re-examination

¹ Italics are the speaker's.

of the preconceptions of the science. A second is the attempt to free theory from its ethical implications and give it a more positive statement. This is evidenced by the tendency away from "marginal utility" and from the "productivity" theory of distribution. A third is a disposition to replace the static discussion of doctrines, whose objective has been eternal verity, with a consideration of systems in their larger aspects against the background of their economic and intellectual environment. The attempts at constructive work are as varied. Quite significant is the noteworthy attempt to restate the assumptions of economics. The recent introduction of the "new volitional psychology" and the substitution of the idea of "process" for that of "normality" are striking examples. Equally significant is the attempt to cover the generally neglected field of institutions. Professor Mitchell has just advocated the introduction of one of these, pecuniary valuation and motivation, in value theory itself. Others are insisting upon a critical study of the institutions which condition the valuations of the market-place, such as property, competition, and even pecuniary valuation itself. Mention must also be made of the reappearance of "welfare" as the fundamental concept of the science. This idea has played quite a part in English economics in the last few years.

What tendencies among these will prove of positive value, what contributions they will lead to, and what uses will be made of them, who can say? They furnish, however, sufficient evidence that theorists appreciate the necessity of observation and that they are intent upon relevancy. They will assuredly make some contribution to economic theory. But as surely we know that much of the old is valuable. In logical consistency it sets an ideal which economics must ever keep before it if it would preserve its wonted intellectual respectability. To be contemptuous of speculation and generalization or of observation is alike dangerous. To scorn the old or to venerate it is alike inimical to advance. In the future, as in the past, by aiming at relevancy and consistency, by using speculation, observation, and generalization, economic theory must work its way from tentative statement to tentative statement.

RICHARD T. ELY: The discussion in some of its aspects sounds to me like a voice from the tombs. Professor Hollander goes back to a distant past and reminds us of sins which we committed long ago. But not all those things which may now seem like sins were sins after all. Utterances of the past must be judged with respect to the conditions of the past. My friend and colleague, Taussig, and I can look

back a good many years, and I am sure that he feels, as I do, that great progress has been made since the American Economic Association was founded. Theory has been improved in a good many respects. However, this is a large subject, and I hope that on some future occasion I shall also have an opportunity to read a paper on economic theory and then I can have my say too.

IRVING FISHER: Both of the papers seem to me excellent. I would like to say a few words about Professor Hollander's. His is the best paper on methodology which I have heard before this Association or elsewhere. I admit, however, that this is not necessarily as high praise as it sounds!

I was somewhat disappointed at the reaction of those who commented on Professor Hollander's paper. I sincerely hope that the young men who are beginning to work in economic theory will take to heart what Professor Hollander has said. I have long believed that economists would profit greatly by taking some older and more developed science as a model for method; it does not much matter whether it be physics, biology, or some other science. One of the speakers has said that economics is not physics. No, but its method is the method of physics, and I believe a study of physics to be one of the best preparations for a young man intending to enter economic theory. The trouble with economic theory is that economists have entered the field, either from the *a priori* side of philosophy and metaphysics where the proper importance of cold facts has not been recognized, or on the other hand, from the side of history where only facts and not principles have been studied. The result is that we have suffered from both of the evils against which Professor Huxley warns all scientists. My chief criticism of Professor Hollander's paper is that he considered only one of the two pitfalls mentioned by Huxley. He complained of half-baked theories. He should also complain of half-digested facts. Our theoretical economists are not sufficiently practical, while our practical economists are not sufficiently theoretical. I might also raise the question whether Professor Hollander has not been unduly severe with some of the individuals whom he has selected to criticise. Some of them, at least, were doubtless unable to supply the needed verification. He has given us a counsel of perfection. Often in economics as in any other science we have to throw out suggestions or hypotheses in advance of any possibility of verifying them. We can scarcely be criticised if we realize that verification is needed and so specify. Severe criticism is merited only by those, and there are many, who fail to recognize that verification is even desirable.

B. M. ANDERSON, JR.: I shall take time only to enunciate a thesis, in further development of the theme of Professor Mitchell's excellent paper on "The Rôle of Money in Economic Theory."

Very much of our economic theory has been static theory, concerned with "normal equilibria" and delicate marginal adjustments, resting on the assumptions of a fluid market where labor, capital, and goods are perfectly mobile. Theory resting on these assumptions may, indeed, abstract from the idea of money. If these assumptions were true, money would, indeed, be a meaningless "cloak," obscuring the real forces at work. This body of static or normal theory has real significance for the understanding of economic life. The tendencies it formulates are real. The marginal adjustments are made, not as smoothly as the pure theory would indicate, but none the less smoothly enough to give verisimilitude to the theory.

But the explanation of the degree of fluidity which the market manifests, of the ease and promptness with which adjustments to changed conditions are brought about, is to be found, in large measure, in money and credit themselves. It is the business, the *function*, of money and credit to overcome "friction," to bring about quick readjustments, to effect "transitions," to make possible the static equilibria,—in a word, to make the assumptions of static theory come true. This is not the whole story as to the functions of money and credit, but it is the central point. The greater part of the exchanging that actually takes place in the modern world grows out of dynamic changes in economic life. In a static state, or normal equilibrium, where every laborer has already found the work that is the best he can get, where every capitalist has already found the investment which suits him best, where land and houses are in the hands of those best adapted to hold them, etc., there would be little occasion for the sale of lands, of corporate securities, etc. With the price fluctuations due to the uncertainties of transition periods and dynamic changes banished, there would be little occasion for speculative trading. Speculative exchanges today make up the major part of the actual volume of trade in the United States.¹ In a state of normal equilibrium there would be, of course, exchanges for the purpose of carrying raw materials through the various stages of production to the consumer, etc., there would be payments to laborers, etc., and there would be some "time speculation" in articles where there is seasonal variation in production. But the great bulk of the actual exchanging which now takes place would not occur.

Static theory thus rests on assumptions which are valid chiefly be-

¹ I shall offer statistical evidence on this point in a book on *The Value of Money* now in process, which I hope to publish during the coming year.

cause money and credit have made them so. Static theory thus rests on the foundation of money and credit. It is somewhat ungracious for static theory to despise that foundation, and very indiscreet for static theory to try to knock the foundation down.

It is, moreover, quite grotesque for static theory to offer itself as a support for its own foundation. A static or "normal" theory of money and credit, resting on the notion of accomplished equilibrium, after transitional changes have been effected, misses the main point as to the function of money and credit. Static theory which assumes frictionless fluidity, misses the whole point concerning money and credit. A functional theory of money and credit must be a dynamic theory, basing itself on an analysis of friction, of transitions, and the like. And this is one reason, among many, why I find the quantity theory of money indefensible.